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WHAT HAPPENED TO PATROL OPERATIONS IN KANSAS CITY? A REVIEW OF THE KANSAS CITY PREVENTIVE PATROL EXPERIMENT[1]

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ABSTRACT

This paper reviews important aspects of the design, execution, and evaluation of the Kansas City Preventive Patrol Experiment. The focus is on the operational behavior of the patrol force during the experiment and not on before-and-after crime statistics. Where appropriate, simple probabilistic models are employed to estimate frequencies of preventive patrols and response times in each of the experimental areas. These models, together with experimental data, demonstrate that (1) typical patrol intensities in Kansas City are not large enough to encompass the range of patrol intensities experienced in other cities, and (2) patrol visibility in the depleted areas (the reactive beats) due to responding calls for service is relatively quite large, perhaps even equalling the pre-experimental levels during high workload periods. Such models also demonstrate that travel distances into the reactive beats should not be markedly increased, as the researchers had expected.

Based on models and experimental data, the analysis indicates that the particular experimental design used in Kansas City resulted in a significant continued patrol presence in the depleted areas, with little increase in travel times in those areas. This suggests two policy conclusions: (1) great caution should be used in attempting to induce the general value of a visible patrol presence from the results of the Kansas City Preventive Patrol Experiment; (2) patrol administrators in other cities could, on a day-by-day basis if need be, remove conventional patrol coverage from certain beats and markedly increase manning in others nearby without incurring significant degradations in service (either actual or perceived) in the depleted areas. This second possibility facilitates the implementation of crime-directed patrol efforts.

I. INTRODUCTION

The Kansas City Preventive Patrol Experiment (Kelling et al., 1974) is by now well known for its attempt to test the effectiveness of traditional preventive patrol, at least as practiced in the Kansas City Police Department.

Briefly reviewing the experiment, fifteen patrol beats comprising a nearly rectangular area in the twenty-four beat South Patrol Division were selected for the experiment by the researchers and a task force of police officials. Of these fifteen beats, five were selected as control beats; that is, preventive patrol was to continue "as usual" in those beats. At the core of the experiment, five were selected as reactive beats, in which (according to the authors) ". . . there would be no 'routine preventive patrol' as such. Cars assigned these beats would enter them only in response to calls for service. Their noncommitted time . . . would be spent patrolling the perimeter of their beats or the adjacent proactive beats" (Kelling et al., 1974:28). (According to the technical report, "perimeter" here means literally the streets comprising the beat boundaries.[2]) The proactive beats, which were the remaining five beats, were to display (according to the authors) ". . . two to three times the usual level of police patrol visibility through the assignment to these beats of extra patrol cars and through the presence of cars from the reactive beats" (Kelling et al., 1974:28). It is important to note that the increase in patrol levels in the proactive beats is due to two causes: (1) the patrol in these beats (at least near certain borders) of one or more reactive units and (2) the assignment of a second full-time unit (in addition to the regular beat unit) to each proactive beat; thus, on average, each proactive beat had two full-time proactive units assigned plus the limited patrol coverage of one reactive unit. By this experimental design, an attempt was made to vary police visibility from minimal (reactive) to same-as-usual (control) to "two-to-three" times same-as-usual (proactive).

In selecting the three different experimental conditions, the beats were matched in groups of three to reduce disparities in ethnicity, income, crime incidents, residential permanence, and rates of calls for service. In addition, the judgment of task force members and other police officials was used to override the computer-derived optimal matching of beats in order to take into account idiosyncrasies of various beats (e.g., privately patrolled shopping centers). This judgment also resulted in a spatial dispersement of the reactive beats. A fact that is very important when considering the results of the Kansas City Experiment, *each reactive beat shared a beat boundary with at least one proactive beat and at least one control beat.* No two reactive beats shared any boundaries in common.[3]

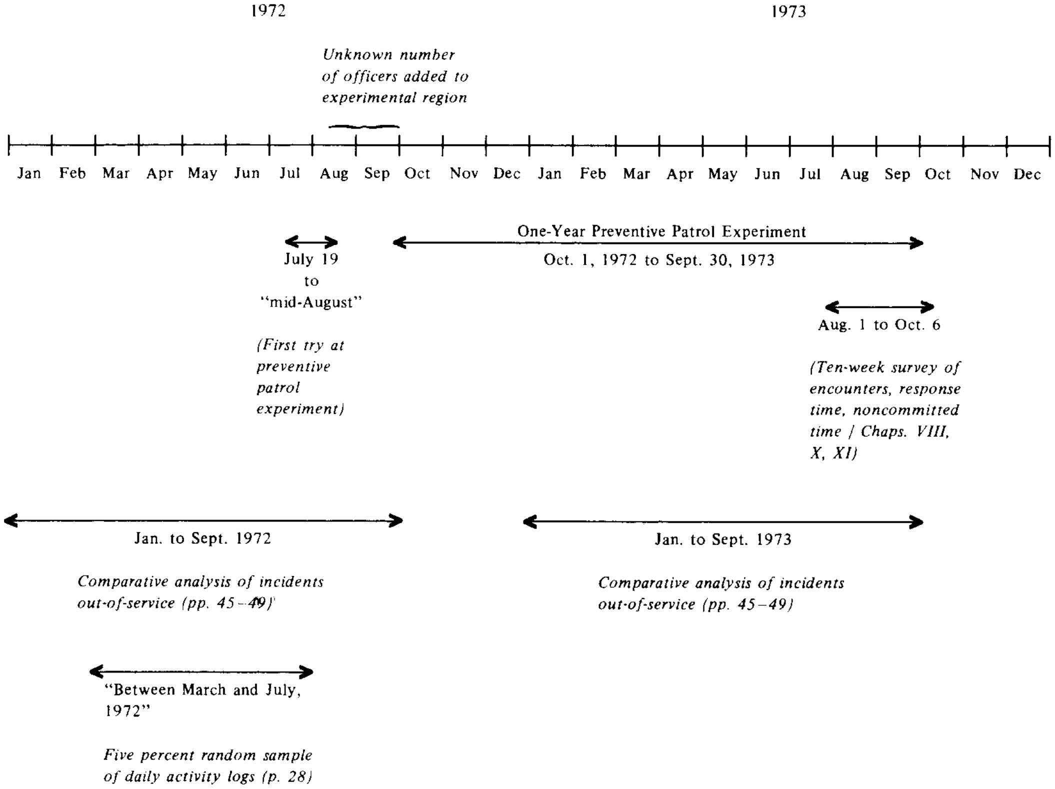
The experiment was run for one year – from October 1, 1972 to September 30, 1973. To assist in our discussion, a chart of the key time periods of the experiment is given in Figure 1. (Each entry in Figure 1 will be discussed below.)

The researchers who designed the experiment and authored the final report are to be congratulated on a candid and frank discussion of the administrative, bureaucratic, and interpersonal problems that accompanied the experiment (and are likely to besiege any urban sector experiment of this magnitude and scope). These problems are not only discussed in Chapter IV of the technical report, but also later by one of the report's authors (Brown, 1975).

The researchers are also to be credited for bringing an impressive array of surveys and analyses to bear on the evaluation of experimental outcomes. Included were surveys of community attitudes, surveys of local businessmen's attitudes, analyses of police-citizen interactions ("encounters" and "transactions") as reported by observers, analyses of crime rates (both reported to police and reported via a victimization survey), analyses of police non-committed time (which have value independent of the experiment), analyses of rates of traffic accidents, and surveys of police officer attitudes regarding preventive patrol. Each should play

FIGURE 1

KEY TIME PERIODS IN THE KANSAS CITY PREVENTIVE PATROL EXPERIMENT



Source: Kelling et al., 1974.

an important role in evaluating preventive patrol experiments such as this one. And their testing in the Kansas City environment will enable others to build on this first experience in order to conduct related experiments in other cities.

In a way, the seed effect of the Kansas City Experiment may be its most important contribution. The experiment has sparked a lively debate in police circles and has prompted others to want to test their own hypotheses regarding preventive patrol in their own cities. Thus, acceptance of hypothesis generation, experimentation, and other aspects of the scientific method is surely a valuable contribution of the Kansas City Preventive Patrol Experiment.

II. FOCUS OF THIS REVIEW

It is especially critical that evaluations of the Kansas City Preventive Patrol Experiment are undertaken since the experiment is being marketed as a definitive work which questions the utility of traditional preventive patrol. In this light it is important to examine the design of the experiment to see whether the conditions expected were actually brought about. In addition,

one must examine the conduct of the experiment itself to see if experimental conditions were maintained. Finally, it is necessary to look at the types of data used in evaluating the final outcomes of the experiment. The following sections address each of these issues while examining, one-by-one, specific topics emphasized in the technical report. Where appropriate, liberal use is made of back-of-the-envelope models of patrol operation.

At the crux of the entire exercise lies the question, What are the policy implications of the Kansas City Preventive Patrol Experiment? Without delving into details here, the researchers found primarily negative results; that is, few of the many measured quantities (such as reported crime rates, victim-derived crime rates, citizens' perceptions of police presence and service, response times, etc.) varied in a statistically significant way by experimental area. Since the focus of the experiment was on preventive patrol, and since the experiment yielded basically negative results, one is tempted to think that the experiment implies that patrol manning levels could be decreased (thereby reducing time available for preventive patrol) without incurring degradations in patrol performance (actual or perceived). In this regard, it is important to note that the entire fifteen-beat experimental area in Kansas City did not incur a net decrease in patrol manning, but to the contrary, it experienced an increase. The Kansas City Experiment focused on a *spatial redistribution* of patrol resources within a confined region (the fifteen-beat area); it did not examine the effect of a net reduction of patrol effort within the region. Thus the policy implications are likely to be constrained to questions relating to spatial redistributions of patrol resources, not to net increases or decreases of patrol resources.

Yet one might interpret the three types of experimental areas – reactive, proactive and control – to represent three semi-isolated areas that can be treated independently. And if patrol performance in the depleted area is not noticeably degraded compared to the other areas, then one could conclude that preventive patrol, per se, has little or no value. The resolution of this issue is a major concern of this review. Far from being independent areas, the three types of beats comprise a closely intertwined spatially distributed service system. For instance, it is shown that the depleted areas can draw resources from nearby heavily manned areas to handle calls for service with nearly no increase in travel time. And the very act of responding to calls for service within the depleted areas retains a patrol visibility that is often close to that experienced previously.

The patrol force, as a service system, is a key component of the police emergency response system, which also consists of telephone complaint clerks, dispatchers, and strategies for their operation. Queuing delays at the dispatcher's position (due to all units being simultaneously busy) are very dependent on the number of patrol units fielded. From a queuing point of view, preventive patrol (or an equivalent activity that can be interrupted for response to calls for service) provides required residual time that keeps the probability of lengthy delay below some target value. Rates of calls for service, dispatching policies, on-scene service times, response and patrol speeds – all of these descriptors are required to understand the behavior of police emergency response systems. Thus, one aim of the review is to examine the planning and documentation of the Kansas City Experiment with respect to adequacy in this area. (An underlying assumption here is that the documentation should stand alone – much as the documentation of any scientific experiment.)

Another aim is to demonstrate that one should not use the results of the Kansas City Experiment to extrapolate the general value of a visible patrol presence. The range of patrol coverages in the three experimental areas simply was not large enough to do this. We are all aware of the impacts of extremes in patrol coverages – for instance, the crime wave which accompanied the 1919 Boston police strike or the marked reduction in crime that could occur from police on every street corner. But most police departments operate at intermediate points

on the "curve," where it is difficult to measure the impact of, say, a fifty percent increase or decrease in coverage. And even a doubling of coverage in Kansas City may not come near a typical level of coverage in another city – say New York, Boston, or Washington, D.C.

A third and final aim of the review is to describe the major positive policy implication – in this reviewer's opinion – of the Kansas City Experiment, namely, that patrol administrators in other cities – if they proceed with care, as was done in Kansas City – can be quite flexible in redistributing their resources spatially within a confined region. This conclusion, which seems to emerge as an unexpected by-product of the experiment, allows implementation of crime-directed patrols in high crime areas, and the locations and strategies of such patrols could change on a day-by-day or even hour-by-hour basis. The need for such focusing of patrol efforts is amply demonstrated in the Kansas City Report, which revealed that aimless, purposeless patrol resulted in time spent on many nonpolice and trivial activities.

To assist in the following discussion, Appendix 1 contains a summary of technical definitions, and Appendix 2 contains a summary of the key idea of each equation.

III. LACK OF DESCRIPTION OF DISPATCH PROCEDURES

Two primary activities of an urban police patrol force are (1) responding to calls for service and (2) performing preventive patrol.[4] The "call-for-service activity" is what ordinarily interrupts preventive patrol.[5] And, in the Kansas City Experiment, it is the presence of cars responding to calls for service within the reactive beats that raises the patrol visibility above zero.[6]

Since call-for-service activity is such a crucial part of the total activity of a patrol force, it is surprising that the Kansas City researchers did not pay more attention to it. In particular, there is no description of the patrol dispatching procedures in Kansas City. In one of the few references we see to dispatching, the authors state, "There was no attempt to adjust departmental procedures for dispatching calls for service" (Kelling et al., 1974:496). On the surface, this is fine as long as what the procedures are is known. Is a car always dispatched immediately if at least one is available, or are certain calls "saved" for the beat car? Under what circumstances are two units dispatched to the scene of an incident? What is the call priority structure?

It is stated that the South Patrol Division has twenty-four beats, but only fifteen were selected for the experiment. In this reviewer's experience, one dispatcher would handle calls from all twenty-four beats (and it is reasonable to presume this unless told otherwise). Thus, it appears that if the fifteen-beat experimental area is to remain isolated, dispatching procedures would have to be changed – making a fifteen-beat cooperating region and a nine-beat region.[7] If this was not done, how were intrusions of cars from one region into the other accounted for in the experimental design and in the evaluation statistics? If this was done, what were the instructions given to the dispatchers?

Whatever the resolution to this problem is, there remains the inescapable fact that due to the change in the spatial distribution of the patrol force, the dispatch procedures *had to* change. With additional units in the proactive beats and reactive units patrolling beat perimeters, the dispatcher's "guesstimated" locations for units must change – and therefore the order in which he prefers them for calls from each location will change (in general). Such changes affect travel times, patrol workloads (on calls for service), numbers of cross-beat dispatches, and other factors that bear on the Kansas City Experiment. For an experiment with such elaborate evaluation tools in other areas, it is surprising (and disappointing) that we are given no information in this area.[8]

IV. NUMBER OF ASSIGNED UNITS

The discussion of manning levels in the experimental area is limited to seven sentences and one chart in Chapter III. From this we surmise that each of the three eight-hour watches (tours of duty) had fifteen beat officers assigned, five support officers (to provide the second regular unit in each proactive beat), and four lab and wagon officers. The average manpower shortage per watch (based on a sample of 193 watches) was typically only 1.0 to 1.5 units per watch.[9] Of significance here is the fact that manning levels *were held constant* over twenty-four-hour periods. Unlike some departments which schedule manpower in accordance with call-for-service workloads and other anticipated police activities, Kansas City utilizes equal manning over twenty-four-hour periods. This means that the majority of preventive patrol activity occurs during the early morning hours when citizens are asleep and therefore unable to perceive changes in preventive patrol levels. It also means that the least amount of preventive patrol occurs during the high workload hours – typically from 1600 to 2400 hours, when citizens are not at work and best able to perceive changes in preventive patrol coverage. (These facts will play a role in Section VIII, which examines preventive patrol coverage in detail.)

Since changes in citizens' perceptions are being measured, it would seem important to discuss manning levels in the experimental area *prior* to the experiment. The authors hint that low manning levels forced the premature stop of the experiment, which had first been started on July 19, 1972, but had to be suspended in mid-August. According to the evaluators, "One problem was manpower, which in the South Patrol Area had fallen to a dangerously low level for experimental purposes. To meet this problem additional police officers were assigned to the area and an adequate manpower level restored" (Kelling et al., 1974:32).

One must ask the question, What is meant by "dangerously low level for experimental purposes?" The reader is led to suspect that the South Patrol Area operated "as usual," except for the two primary experimental conditions of additional cars in the proactive beats and no patrolling of the reactive beats. However, this statement suggests that "operating as usual" meant that not all beats could be covered and therefore that the experimental conditions created some anxieties on the part of experienced patrol supervisors. To correct for this, it appears that additional units were brought into the experimental region. The full-scale twenty-four-hour manning is fine for maintaining the experimental conditions, but what effects would result if, say, only ten units had been available for the fifteen-beat experimental area prior to the experiment? (This reviewer has found such situations the rule rather than the exception in other cities.) Then, the virtual doubling of visible patrol units in the region would almost certainly heighten a citizen's awareness of police presence (even for citizens in the reactive beats, who probably travel into or through other beats in the region). It would tend to *reduce* response times, since the likelihood of congestion would be greatly reduced. It would reduce a patrol unit's call-for-service workload, since the workload would be shared among twice as many units, and thereby increase the time for preventive patrol. A lack of discussion of prior manning levels represents a serious omission.

V. WHAT IS THE CALL-FOR-SERVICE WORKLOAD?

Statistics describing the activities of the patrol units are often presented in a confusing and sometimes incomplete manner. This is unfortunate since it diminishes somewhat the utility of the unique analysis of officer-noncommitted time.

For the purposes of comparing Kansas City to other cities, it is critical to obtain a clear idea of the call-for-service workload within the experimental area. Yet this reviewer found this

to be a very difficult thing to do from reading the technical report. Some key information is presented in a very confusing paragraph on page 28, which summarizes "... a five percent random sample of daily activity logs of all officers assigned to the experimental area conducted between March and July of 1972 ..." One may ask why the sample was taken during this time. Since the experiment itself was started in mid-July (the last month of the five percent sample period), this was a time when patrol manning had fallen to "dangerously low levels for experimental purposes," thereby necessitating suspension of the experiment in mid-August. Also, since most of the five percent sample took place *before* any increase in manning (the added units in the proactive beats presumably accounted for one increase on July 19) and all of it took place before the region-wide increase (which accompanied the restart of the experiment on October 1, 1972), we should not accept the figures from the five percent sample as representing the way in which units spent their time during the conduct of the experiment. Instead, the figures represent activity profiles during a period of "dangerously low" manning levels.

Second, the data are presented in a confusing way which combines (needlessly, in this reviewer's opinion) both in-service and out-of-service activities. The report is deficient in defining precisely the terms used throughout – terms that have meanings in Kansas City that are most likely to differ in some respects from their uses in other cities. After reading the report, this reviewer's interpretation of out-of-service time is that time which is committed to activities that are known by the dispatcher. These include calls for service, certain self-initiated activities, certain assignments, etc. "In-service" relates to time spent on activities that are unknown to the dispatcher, hence to time that the dispatcher believes the car is available to respond to calls for service. A key in-service activity is routine preventive patrol. The authors themselves seem to question the validity of these data: "The total does not equal one hundred percent due to unaccounted for or improperly accounted for time" (Kelling et al., 1974:28). As a result of the five percent random sample officers were found to be spending only 18.54 percent of their time on calls for service. Yet on this same page we are told that the department had previously estimated that sixty-five percent of a patrol unit's time is committed to calls for service. This is a huge discrepancy – yet there is no explanation given for it. While the latter figure may have been a seat-of-the-pants guess, unfortunately it was "... considered adequate for computational use" (Kelling et al., 1974:28) in initially setting up the experimental design.

In order to obtain an independent estimate of the fraction of time spent on calls for service, this reviewer used the annual calls-for-service data given on page 50.[10] Here, for the period of the experiment, it is estimated that the experimental region generated a total of about 45,000 calls for service (Kelling et al., 1974:50, averaging). This averages to 123 calls for service per day or about eight calls for service per beat per day. But the average time at the incident is estimated (by citizens[11]) to be twenty minutes (Kelling et al., 1974:494), so adding roughly ten minutes for travel to and from the scene, the average service time per call for service is unlikely to exceed thirty minutes. Thus, in minutes, the average call-for-service workload per day per beat is not likely to exceed $8 \times 30 = 240$ minutes, or four hours. This represents a fraction of time handling calls for service equal to 16.7 percent (assuming one unit assigned full time to each beat). Even adding the possibility of multiple responses to calls, it is unlikely that this figure would climb more than a couple of percentage points. So the 18.54 percent reported from the five percent sample is not an unreasonable estimate. Since the 18.54 percent estimate was reported for a period of "dangerously low" manning levels and since these estimates of call-for-service volume were taken from a table that included a few activities other than calls for service, the low figure of 18.54 percent is most likely an overestimate of the

call-for-service workload in a full-manning situation. The addition of five more units to the proactive beats during the experiment pulled the average call-for-service workload down even further.

A key reason for this low average is that Kansas City utilizes equal manning on each of the three eight-hour tours of duty. Thus, while the call-for-service workload during evening hours may be significantly greater than 18.54 percent, the early morning workload is likely to be very small. Thus, the dominant time spent on preventive patrol is the early morning hours. This is a time when the need for preventive patrol is probably least, but – by the design of the preventive patrol experiment – a time when the intensity of preventive patrol in Kansas City was at a maximum. In assessing the applicability of the Kansas City Experiment to their own situations, patrol administrators in cities that experience higher call-for-service workloads (per car) – most likely as a result of scheduling patrol officers in a way which reflects the twenty-four-hour pattern of calls for service – must take into account these special attributes of the Kansas City Experiment.

VI. NONRANDOM SAMPLING BY PARTICIPANT-OBSERVERS

The evaluation utilized participant-observers who rode with police officers and gathered data on various aspects of the experiment. These observers were utilized heavily in obtaining the results of three chapters: VIII, "Police-Citizen Encounters"; X, "Police Response Time"; and XI, "Analysis of Police Noncommitted Time." The observers rode in police cars during the ten-week period August 1, 1973 to October 6, 1973. (One can reasonably ask why the last six days of this period extended beyond the cutoff date of the experiment, September 30.)

Given the focus on certain quantitative aspects of the experiment, this review will not dwell on the possible limitations of use of participant-observers. (The authors themselves discuss these issues.) However, there is a subtle biasing in the way in which the observers were assigned to cars – and this could have affected certain outcomes of the ten-week study. In Chapter XI (the third of three chapters reporting results of the ten-week study) we are finally told that the participant-observers scheduled their in-car hours for *high activity beats.*

> The sample of officers used was the same as developed for the response time and encounter surveys. This sample represented those officers and beats which, in the judgment of the observers and task force members, had the highest levels of activity (calls for service, car checks, pedestrian checks, etc.). As such, the sample biases the following data in the direction of committed time; the noncommitted time can be considered to have been at a minimum level (Kelling et al., 1974:500).

This selection of high activity areas results in a very nonrandom sample for Chapters VIII, X, and XI. While it is unclear how this lack of randomness would affect police-citizen encounters,[12] it is clear that police noncommitted time would tend to be lower than region-wide averages and that response times could be affected. Response times would tend to be higher (due to heavier workloads and thereby greater numbers of cross-beat dispatches); this effect may be offset to some extent by the (presumably) smaller areas of the heavy workload beats. Whatever the net effect, it is clear that response times and cross-beat dispatches could be measurably biased compared to region-wide averages.

VII. LEVEL OF PREVENTIVE PATROL

The researchers claim that preventive patrol visibility was significantly curtailed in the reactive beats, stayed the same in the control beats, and increased two to three times in the proactive beats. There are several reasons why we should seriously question these claims. To do this, we must estimate quantitatively some measure of the level of preventive patrol.

A. *Preventive Patrol Equation*

It is not difficult to derive an equation which allows us to examine the level of preventive patrol by predicting the *frequency of patrol passings* (in number of passes per hour) in terms of simple quantities. Not only is the concept of preventive patrol frequency useful in itself – by allowing one to define precisely a level of preventive patrol – but, once the preventive patrol frequencies are known, one can easily obtain estimates of other important performance measures (such as the probability of a patrolling unit intercepting a randomly occurring crime in progress).

The key quantities which affect the level of preventive patrol in a beat are the following:

$N \equiv$ number of patrolling vehicles in the beat

$s =$ effective speed (in mph) of the unit while patrolling

$b =$ average fraction of time each unit is busy (i.e., not performing preventive patrol)

$K =$ number of different levels of preventive patrol given to the street segments in the beat

$e_k =$ relative amount of patrol coverage given at the k^{th} level of preventive patrol ($k = 1, 2, \ldots, K; 0 \leqslant e_k \leqslant 1$)

$\ell_k =$ number of street miles in the beat given the k^{th} level of coverage.

Intuitively, we expect the level of preventive patrol to increase linearly with the number of patrolling units, N, and the effective speed, s. If b is the fraction of time that units are busy (i.e., not performing preventive patrol), on the average, then $(1 - b)$ is the fraction of time they are performing preventive patrol. Thus, we also expect the level of preventive patrol to increase linearly with $(1 - b)$.

As for K, e_k, and ℓ_k, we illustrate their interpretation and use by example. Suppose that four different patrol levels are given to street segments in the beat:

Level	*Relative Coverage*	*Street Miles Receiving This Coverage*	*Type of Area*
1:	$e_1 = 1$	$\ell_1 = 1$ mile	Storefronts
2:	$e_2 = 0.5$	$\ell_2 = 3$ miles	Other commercial zones
3:	$e_3 = 0.25$	$\ell_3 = 10$ miles	Major residential streets
4:	$e_4 = 0.1$	$\ell_4 = 11$ miles	Minor residential streets; misc. alleys

Then we would expect the level of preventive patrol to vary linearly with e_k; for instance, storefronts in this example would receive ten times the amount of preventive patrol that minor residential streets and miscellaneous alleys would receive.

The final concept we need here is that of *effective patrollable street miles in the beat,* L. The idea behind L is that it represents the average number of miles driven between successive passings of a point in the maximum coverage area (storefronts in this example). After the patrolling unit has passed a point in the maximum coverage area, it will travel an average of (½) X 3 = 1.5 miles in the Level 2 area (since $e_2 = 0.5$), (¼) X 10 = 2.5 miles in the Level 3 area (since $e_3 = 0.25$), and (1/10) X 11 = 1.1 miles in the Level 4 area (since $e_4 = 0.1$); in addition, it will travel an average of 1 X 1 = 1 mile in the maximum coverage area. Thus, we define L to be a weighted sum of the street mileages in the beat, the weights corresponding to the respective coverages given each area:

$$L = e_1 \ell_1 + e_2 \ell_2 + e_3 \ell_3 + \ldots + e_K \ell_K \tag{1}$$

For the example, L = 1 X 1 + 0.5(3) + 0.25(10) + 0.1(11) = 6.1 miles. It makes sense intuitively, and can be shown mathematically (Larson, 1972:136–138), that the patrol frequency would vary inversely with L. Thus, in the example, if L were doubled to 12.2 miles, each type of area would receive one-half the patrol frequency as before (all else remaining constant).

Summarizing this development, our equation for predicting patrol frequency of a level k area in a beat is

$$f_k = e_k \, N \, s \, (1 - b)/L \tag{2}$$

To provide an example of the use of the equation, suppose

N = 1 vehicle

s = 10 mph

b = 0.5

L = 25 miles

e_2 = 0.5

That is, the single patrolling police vehicle averages ten miles per hour, is busy (not performing preventive patrol) fifty percent of the time, patrols a beat with twenty-five effective patrollable street miles, and provides one-half the patrol frequency to Level 2 areas as to Level 1 areas. Then, the frequency of preventive patrol passings in Level 2 areas is

$$f_2 = (0.5)10\,(1-0.5)/25 = 2.5/25 = 0.1 \text{ pass/hour}$$

In other words, the patrol vehicle passes an average point in a Level 2 area 0.1 times per hour or ten times per one hundred hours. Another way of looking at this is that an average of $1.0/f_2 = 1.0/0.1 = 10$ hours are consumed between passings of the patrolling vehicle at an average Level 2 point in the beat. Note that the equation behaves as we expect: If the patrol speed, s, is doubled to twenty miles per hour, then the patrol frequency is doubled (to 0.2 pass/hour),

or the mean time between patrol passings is halved (to five hours). If the fraction of time busy is increased fifty percent from 0.5 to 0.75, then the patrol frequency is halved to 0.05 pass/hour (or twenty hours between passings). If the effective patrollable street miles is halved, changing L from 25 miles to 12.5 miles, then the patrol frequency is doubled (and the average time between passings is halved).

B. Patrol Frequencies in Kansas City

Now, applying these concepts to the Kansas City Preventive Patrol Experiment, it would seem that any experiment focusing on the level of preventive patrol would attempt to measure, or at least to impute, the patrol frequencies (or correspondingly, the average time between patrol passings) in each of the three experiment zones (reactive, proactive, control). Yet this is not done in the technical report, so we must assume it was not done at all. Regarding the parameters of the preventive patrol equation, we are given no information about L (the effective patrollable street miles) or s (the speed of patrol). The speed of patrol could easily have been estimated by using odometer readings recorded in most departments by the police officers at the beginning and end of each tour; in computing the estimate for s one need only know these odometer readings, the time spent on non-preventive patrol activities, and the number of calls for service (all data readily available to the Kansas City researchers).[13]

Based on a very crude analysis of a street map of the experimental area (Davis and Knowles, 1975; Kelling and Pate, 1975a), this reviewer estimates that the average beat contains between twenty-five and thirty total patrollable street miles. For the sake of discussion, average these numbers and say that the average beat contains 27.5 miles. However, one might appear to be stymied since there is no idea of the different levels of coverage given to different areas within each beat; that is, the coverage numbers $e_1, e_2, \ldots, e_K$ are not known. This problem can be circumvented by considering the beat-wide average patrol frequency f,

$$f = N\, s\, (1-b)/D \qquad (3)$$

where D is the total street mileage in the beat (27.5 miles in this case). Here f is the frequency of preventive patrol passings at a *randomly selected* point in the beat, the probability of selection of any particular point being proportional to the coverage number e_k associated with that point. In other words, f is a weighted average frequency, where heavier weights are given to more heavily patrolled areas (Larson, 1972:140–141).

Since no data are given regarding s, the speed of the patrolling vehicle, some means must be found for obtaining a crude estimate. In other cities s has been found to average considerably less than ten miles per hour, usually six or seven miles per hour. For instance, a radio motorized patrol (RMP) vehicle in New York City was known to travel between approximately twenty and forty miles on preventive patrol during an eight-hour tour.[14] Allowing, say, fifty percent time on preventive patrol, this means that the twenty to forty miles were driven during $(0.50) \times 8 = 4$ hours, thereby yielding an average effective speed of the patrolling vehicle of five to ten miles per hour. The actual computed average in New York City was 6.8 m.p.h. (Larson, 1971). For the sake of this discussion, set s slightly higher at $s = 7.5$ m.p.h.

For the last parameter, an estimate of b is required, the fraction of time busy (not on preventive patrol). In the technical report the term $(1-b)$ is known as "uncommitted time" and is said to equal approximately sixty percent (Kelling et al., 1975:500); thus $b \simeq 0.40$. This value was found by the researchers to apply to all three types of units: reactive, proactive, and

control. If one sets $(1-b) = 0.60$, then units are assumed to have averaged sixty percent of their time on preventive patrol. However, the researchers' very valuable analysis of police non-committed time (Kelling et al., 1974:501) indicates that roughly fifty percent of noncommitted time is stationary, not mobile.[15] Thus, for purposes of computing patrol frequencies, it is probably more reasonable to be conservative, setting $(1-b) = 0.30$, which reflects a good deal of time spent at stationary locations. It is clear that setting the value at 0.60 would tend to predict greater frequencies of preventive patrol than setting it at 0.30.

Summarizing, for the preventive patrol equation, the following (not unreasonable) data estimates are obtained:

$$\begin{aligned} s &= 7.5 \text{ mph} \\ D &= 27.5 \text{ miles} \\ 1-b &= \text{fraction of time on preventive patrol} \\ &= \begin{cases} 0.60 & \text{(liberal estimate)} \\ 0.30 & \text{(conservative estimate)} \end{cases} \end{aligned}$$

Utilizing the *liberal* estimate, the average frequency of preventive patrol passings *in the control beats* is found to be

$$f = 7.5\,(1 - 0.40)/27.5 = 0.164 \text{ patrol passes/hour}$$

or about 6.1 hours between patrol passings. The conservative estimate would yield half as much patrol, or about 12.2 hours between patrol passings. Thus, even if the estimates giving rise to these figures are considerably in error (say, plus or minus twenty-five percent), the key result is that preventive patrol "as usual" in the experimental zone is not very much preventive patrol. On a typical tour, using the liberal estimate, a citizen at a randomly chosen spot[16] in a control beat might see a patrolling vehicle pass him an average of once every six or so hours. If his eyes are directed toward the street only ten percent of the time, he would see a patrol car about once every sixty hours.[17] Thus, removing patrol coverage to achieve minimal preventive patrol frequency might be difficult to perceive for the average citizen. It would be somewhat more easily perceived by persons in business in the experimental area who claimed to see police slightly more frequently than once a day prior to the experiment (Kelling et al., 1974:41).

In the proactive beats it is stated that patrol coverage is increased to two to three times the usual levels. If this is true, the frequency of patrol passings increased up to 0.33 to 0.49 passes per hour (using the liberal estimate). This level, it is suggested throughout the report, represents an unusually high amount of preventive patrol coverage. The associated times between patrol passings range from about three to about two hours. Thus, even at three times the usual preventive patrol coverage, the average point is passed by a patrolling vehicle only once each two hours. A major observation here is that this allegedly heavy level of preventive patrol is considerably less than the amount allocated in many cities on a typical day. For instance, the same formula applied to New York City (using much more precise data estimates) showed that in 1969, twelve out of twenty representative precincts experienced greater amounts of preventive patrol during the Saturday evening tour. This is particularly remarkable since that tour is usually the busiest one during the week. (Four of the New York City precincts experienced more than one patrol passing per hour, on the average (Larson, 1972:159–62).) Thus, one is forced to reach the following conclusion:

The range of preventive patrol coverages experienced in the Kansas City Preventive Patrol Experiment is not broad enough to include the range experienced by other U.S. police departments.

VIII. OTHER FACTORS AFFECTING PATROL VISIBILITY

At the crux of the experiment is the claim that patrol visibility was reduced to minimal levels in the reactive beats. The authors acknowledge that inadvertently reactive units would occasionally wander into these beats. Also, the units were authorized to enter the beats "... to apprehend a wanted suspect at a known location, for house checks (with house-to-watch slips), and for all warrant checks except traffic warrants or judgement orders." It is also said that "Reactive Cars may enter their districts for E-calls."[18] And, instructions were given to reactive unit officers to record all entries into their beats onto their activity logs.[19] Yet this reviewer can find no analysis or even discussion in the technical report of these types of entries on the activity logs.

A. *Patrol-Initiated Activities*

There is, however, a very revealing analysis on pages 45–49 of self-initiated patrol activities by experimental zone.[20] These activities consist of the following: (1) traffic violation; (2) assignment; (3) building check; (4) car check; (5) foot patrol; (6) warrant or subpoena; (7) car chase; (8) listing; (9) pedestrian check; (10) other; (11) residence check; (12) assist motorist. Of these twelve categories, it would seem that the following would result from randomly patrolling an area and initiating activities to prevent or deter crime or to function as a traffic officer: traffic violation, building check, car check, pedestrian check, residence check, assist motorist. Thus, the level of these six patrol-initiated activities in each area should be a pretty good indicator of the level of preventive patrol in that area. However, these six activities make up ninety percent or more of patrol-initiated activities in each of the three experimental zones in 1972 and 1973.[21] ("Car check" is the most frequent, totaling about forty-five percent of all patrol-initiated activities.) Thus, totals of patrol-initiated activities can be used to get a valid indicator of patrol presence in each area. What is astonishing here is that the number of patrol-initiated activities in 1973 *in the reactive beats* did not decrease (compared to 1972), but it actually increased! The reactive beats accumulated 5,948 patrol-initiated activities in 1972, but 6,057 in 1973. This is equivalent to about 1,200 per beat or 3.3 per beat per day (slightly more than one per eight-hour tour of duty).[22] The authors make several precautionary remarks about these data, including the following:

(a) Self-initiated incidents comprise only thirty percent of the total out-of-service incidents.

(b) Those activities conducted on the perimeter of a beat are recorded as taking place within the beat.

(c) Some activities may originate outside of a beat and, "due to their inherent mobility, terminate within a reactive beat."

Remark (a) does not diminish the importance of using self-initiated activities as a valid indicator of preventive patrol presence. Remark (b), regarding beat perimeters, is diminished in

importance since only about six miles out of 27.5 (the approximate average street mileage per beat) is on the perimeter of a beat; this represents about twenty-two percent of the total street mileage of the beat. It would be difficult to justify an increased amount of self-initiated activities on only twenty-two percent of the street mileage of the previous year. For those activities which actually did take place on the perimeter, it makes good sense to count them as having occurred within the beat; some people who live in the beat live at its perimeter, and the visible presence of, say, a car stopped with red lights extends into the beat. Remark (c) may be valid for a small minority of incidents, but it cannot explain such factors as a 9.4 percent increase in car checks in the reactive beats (car checks comprising forty-five percent of all reactive beat patrol-initiated activities in 1973). Thus, the analysis of patrol-initiated activities leads us to question seriously the extent to which the experimental conditions were maintained. But there are other reasons, too, as explained below.

B. Specialized Units

More troublesome, other specialized units operated within the reactive beats thereby maintaining police activity (and presumably perceived police presence) considerably above zero. According to the report,

> Since only the effectiveness of routine preventive patrol by the patrol unit was being tested, specialized units (i.e., traffic, helicopter, tactical, etc.) operated as usual. (Such units maintained city-wide mobility and continued to react to specific problems.) However, operation of these units was to remain at a level consistent with levels established for the preceding year (Kelling et al., 1974:29).

Given the experimental focus on regular beat cars, the concept of retaining the specialized units on an operating-as-usual mode is not unacceptable in itself. However, the impressive arsenal of evaluation tools that were brought to bear on other parts of the experiment were not at all directed toward the specialized units. In particular, we have no information about the activity levels of the traffic and tactical units, which could have been increased within the reactive beats due to perceived specific problems or just to anxiety created over the lack of routine police coverage within these beats. The lack of concern over the tactical units, in particular, concerns this reviewer – as is well known, these units can probably be the most visible form of police presence, created by sidewalk interrogations, many police officers in a small area, etc.

But in addition to the intrusions into the reactive beats, by reactive units and the presence of other types of police units, there are other reasons for questioning the validity of the experimental design which attempted to reduce patrol visibility to minimal levels. Four major ones, all interrelated, are as follows:

(1) Responses to calls for service into the reactive beats by the reactive units;

(2) Use of multiple units in responding to calls within the reactive beats;

(3) Increased use of siren and/or lights in responding into the reactive beats;

(4) Peak period activity.

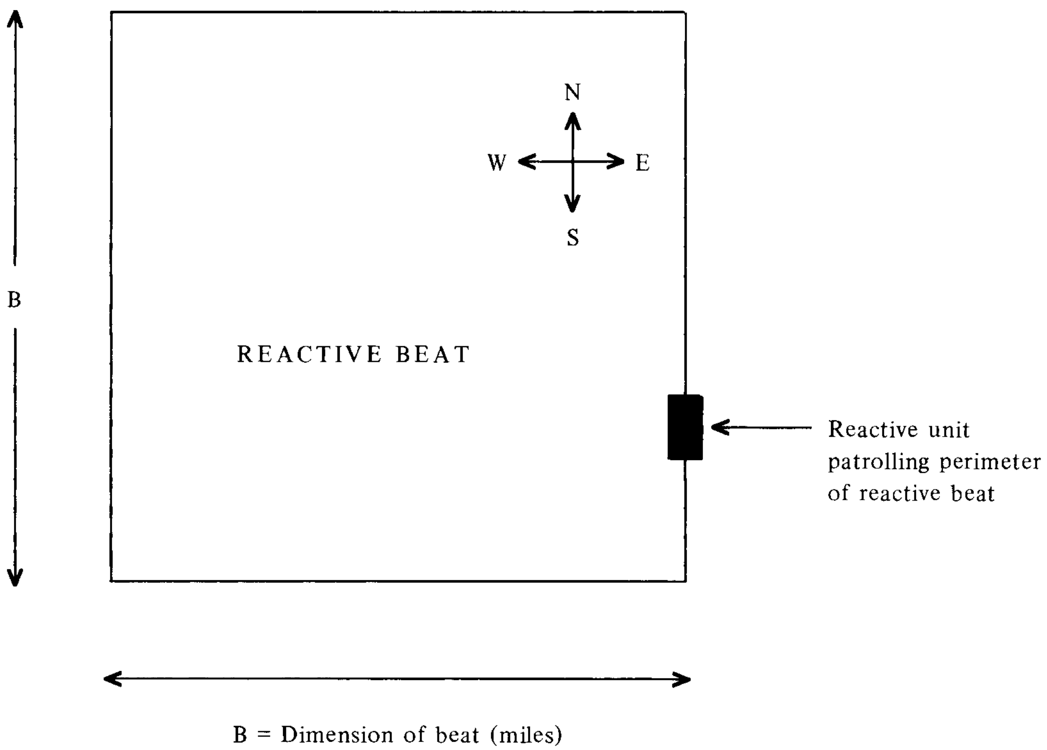
A discussion of each follows.

C. Responding within the Reactive Beats

A diagrammatic depiction of the reactive unit patrolling the perimeter of the reactive beat is given in Figure 2. A simple estimate of the distance the unit will traverse *within the reactive beat* during an eight-hour tour as a result of responding to calls for service is sought. To do this the following simplifying (but not unreasonable) assumptions are made:

FIGURE 2

DIAGRAM OF REACTIVE UNIT PATROLLING PERIMETER OF REACTIVE BEAT



(1) When on patrol, the patrol position of the reactive unit is uniformly distributed over one or more sides of the perimeter.[23]

(2) The location of the call for service is uniformly distributed over the reactive beat and is independent of the location of the reactive unit.

(3) The travel distance is the sum of the east-west distance and the north-south distance between the unit's location (at time of dispatch) and the call-for-service location.

(4) The beat is a square of dimension B (area = B^2).

Given these assumptions, suppose that the unit is patrolling the eastern perimeter. Then

one can show (Larson, 1972:ch. 3) that the average E-W distance to the incident is 1/2 B and that the average N-S distance is 1/3 B; in addition, the responding unit must leave the beat after servicing the incident, incurring an additional average travel distance of 1/2 B (assuming the unit travels in a straight line[24] back to the side it was originally patrolling or to a random side). Thus, the average travel distance travelled within the reactive beat per dispatch there is approximately

$$\bar{d} = \underbrace{\frac{1}{2}B + \frac{1}{3}B}_{\textit{Travel to the incident}} + \underbrace{\frac{1}{2}B}_{\textit{Travel from the incident}}$$

or

$$\bar{d} = \frac{4}{3}B \tag{4}$$

Equation (4) is not dependent on which side of the beat the unit is patrolling at the time of dispatch. In fact, under a remarkable number of alternative assumptions regarding beat geometries and spatial distributions, equation (4) remains a very good approximation for the average distance travelled within the beat per dispatch. In no case has this reviewer been able to generate a set of assumptions that is plausible for the Kansas City situation in which $\bar{d}$ would be less than the beat length B; but he has found not unreasonable sets of assumptions for which $\bar{d}$ would be 5/3 B or more.[25] For the purposes of this review, the plausible range of plus or minus twenty-five percent implied by setting $\bar{d}$ = 4/3 B will suffice.

If N_{CFS} such calls for service are answered per eight-hour tour, then the average mileage travelled within the reactive beat (on calls for service) per eight-hour tour is

$$N_{MI} = \frac{4}{3} B \times N_{CFS} \tag{5}$$

To apply equation (5), B and N_{CFS} must be estimated. The beat dimension B is easy, since the average beat is approximately 2.18 square miles, which can be modelled as a 1.5 mi. X 1.5 mi. square (area totalling 2.25 square miles). Thus, $B \approx 1.5$ miles. The parameter N_{CFS} is also easy since earlier it was estimated that a beat generates an average of eight calls for service per day, or 8/3 = 2.67 calls for service per eight-hour tour.

Thus, using these data estimates, the average mileage travelled within the reactive beat (on calls for service) per eight-hour tour is

$$N_{MI} = \frac{4}{3} \times (1.5)(2.67) = 5.34 \text{ miles}$$

While this may not seem like a significant number at this time, other factors that come into play are now discussed.

D. Use of Multiple Units

The technical report shows that fully 1.58 cars (on the average) responded to each call for

service in the reactive beats. This figure contrasts to 1.29 cars in the control beats and 1.15 cars in the proactive beats. A statistically significant result, the 1.58 figure suggests that police officers were anxious to provide back-up support for their fellow officers responding into the reactive beats. Apparently, many (if not most) of the multiple responses were volunteer assignments rather than directives from the dispatcher (Kelling and Pate, 1975b).

However, the use of multiple responses forces us to modify equation (5), predicting the average mileage travelled within the reactive beat (on calls for service) per eight-hour tour. The new equation is

$$N_{MI} = \frac{4}{3} B \times N_{CFS} \times N_{CARS} \tag{6}$$

where

N_{CARS} = average number of cars responding to each call for service

Applying the Kansas City data, we set $N_{CARS} = 1.58$ for the reactive beats. Thus,

$$N_{MI} = \frac{4}{3} \times (1.5)(2.67)(1.58) \approx 8.44 \text{ miles}$$

So now the result is that during an average eight-hour tour the typical 1.5 X 1.5-mile-square reactive beat is being traversed a total of 8.44 miles by marked police cars responding to calls for service. (It is interesting to note that this figure alone accounts for a patrol frequency of about once a day.)

E. Use of Siren and Lights

Apparently sirens and/or lights are not used very frequently on calls for service in the Kansas City Police Department. In a limited sample, siren and/or red lights were used in only 1.2 percent of the responses in the control beats and 1.0 percent in the proactive beats. However, they were used in 4.9 percent of the responses in the reactive beats. This represents an increase in use of this emergency equipment of between four hundred and five hundred percent. And, due to the fact that these estimates were obtained from in-car observers and that 1.58 cars responded (on the average) to calls for service in the reactive beats, it is likely that siren and/or lights could have been used in as many as (4.9)(1.58) = 7.74 percent of calls from the reactive beats. The true figure is probably somewhere between 4.9 percent and 7.74 percent.

The point here is that this emergency equipment was probably used five or more times as frequently in the reactive beats as anywhere else. Since Kansas City residents were probably not used to hearing sirens or seeing lights on police cars responding to calls for service, such a five-fold increase in use almost certainly would increase citizens' perceptions of the level of police presence.

Thus, the 8.44 miles driven per tour in the reactive beats (on calls for service) was not the typical slow-driven mileage associated with preventive patrol. At least half of that mileage was driven by cars responding hurriedly to calls for service and at least one in twenty calls (perhaps as many as one in thirteen) were responded to with siren and/or lights.

F. Peak Period Activity

All of the calculations in this section have been based on averages which do not reflect variations in police workload by season, time of day, or day of week. Independently of police activity one would expect citizens' perceptions of police presence to vary according to these time factors; this is due to the fact that citizens are more likely to be out of doors during the summer months and on the streets after work hours. Thus, it could be speculated that citizen opportunities for judging police presence are most prevelant during the summer months and during the hours after work.

But during these times police activity on calls for service is at its highest. A not unreasonable rule of thumb is that evening workload (per hour) is twice the daily average (per hour) workload.[26] In Kansas City, it appears from recent annual reports (Kansas City Police Department, 1971–74) that evening workload – averaged over all days of the year – is approximately 5/3 of the daily average. And summer workload (per day) is usually above that calculated as a yearly average; in Kansas City the increase is twenty-five percent (Kansas City Police Department, 1971–74). Thus, during summer evening hours, a workload scale factor equalling conservatively $\frac{5}{3} \times \frac{5}{4} \approx 2.1$ must be applied. (The figure 2.1 is conservative since the time-of-day factor of 5/3 was obtained from hour-of-day statistics for the entire year. Evening increases above the mean daily workload are usually greater during the summer months than during the winter months, thereby indicating that a factor greater than 5/3 is probably applicable to Kansas City during the summer months.)

Assuming all else is constant, then the number of street miles traversed in the *reactive beats* per eight-hour *evening* tour during *summer months* in Kansas City is roughly

$$N_{MI} = \frac{4}{3}(1.5)(2.67)(1.58)(2.1) \approx 17.7 \text{ miles}$$

In fact, due to dispatches which cause a responding unit to pass *through* a reactive beat, this figure could be even higher.[27] However, we will use the figure of 17.7 miles. (Note that if the seasonal and time-of-day correction factor were 3.0, rather than the conservative value of 2.1, N_{MI} would be 25.3 miles.)

Large mileage figures accumulated on calls for service are not unique to the experimental conditions that prevailed in Kansas City. They can also occur under routine (nonexperimental) conditions. For a patrol unit operating as usual (with one patrol unit assigned to each beat), then the average distance travelled in a square beat of dimension B while responding to a call for service is about 2/3 B $(1 + b)$.[28] The other figures used in the above calculation for the reactive beats (1.5 miles for B, 2.67 for the average number of calls for service per eight-hour tour, and 2.1 for the seasonal and time-of-day scale factor) can still be used. However, the average number of cars responding to an incident must be reduced from 1.58 to about 1.22 (the average of proactive and control (Kelling et al., 1974:489)). And b, the fraction of time busy, is at least $2.1 \times 18.5 = 0.39$ and, since in Kansas City other activities out of service typically consume a time roughly equal to the time spent on calls for service, the figure could be as large as, say, 0.78. Employing the lower value for b, an estimate of the number of miles travelled on calls for service within a beat (operating as usual) is

$$N_{MI} \approx \left(\frac{2}{3} + 0.39 \times \frac{2}{3}\right)(1.5)(2.67)(1.22)(2.1) \approx 9.5 \text{ miles}$$

Employing the higher value for b produces

$$N_{MI} \approx (\tfrac{2}{3} + 0.78\times\tfrac{2}{3})(1.5)(2.67)(1.22)(2.1) \approx 12.2 \text{ miles}$$

While these figures are also large – indicating that a substantial amount of patrol visibility in usual patrol beats during high workload hours is due to travel associated with calls for service – it is noteworthy that the particular experimental conditions in the reactive beats in Kansas City caused the usual figure of 9.5 to 12.2 miles to increase to at least 17.7 miles (a percentage increase of eighty-six percent or forty-five percent, depending on the base figure). This increase is due to two factors: (1) the necessity for the reactive unit to travel into the beat and then out of the beat after servicing the incident, and (2) the increased use of multiple units responding to calls for service. In terms of average frequency of passes of the unit, the calculated mileage for reactive beats corresponds to about two passings per three heavy workload tours (while on calls for service) in a beat having 27.5 patrollable street miles.

It is well known that when call-for-service workload is at its highest, routine preventive patrol is at its lowest. During the summer evening tour the typical Kansas City police unit is responding to calls for service approximately thirty-nine percent of the time. Additional "committed time" would easily increase this figure to sixty percent.[29] This leaves at most one hundred minus sixty equals forty percent for noncommitted time. Assuming that roughly half of that is spent on routine (mobile) preventive patrol,[30] that allows about twenty percent time (or 1.6 hours per eight-hour tour) on preventive patrol. Assuming a driving speed of 7.5 miles per hour, this implies that (roughly) (7.5)(1.6) = 12.0 miles could be driven by the unit in performing routine preventive patrol. This compares to at least 17.7 miles being driven within reactive beats responding to calls for service. Even if seventy percent (rather than fifty percent) of noncommitted time is spent on mobile preventive patrol, the mileage driven within the beat on such patrol is not likely to exceed the mileage traversed within the beat when it is made a reactive beat.

Summarizing, a typical beat (operating as usual) under these high workload conditions would experience 9.5 to 12.2 miles driven by units on calls for service plus the 12.0 miles driven on preventive patrol. Adding the two types of mileage together, a total of 21.5 to 24.2 miles driven per tour (under heavy workload conditions) is obtained in a beat operating as usual. But the 17.7 miles calculated for the reactive beats represents little net reduction in miles travelled under these conditions. Even if the total mileage associated with a beat operating as usual were as high as thirty or thirty-five miles (rather than 21.5 to 24.2 miles), the figure of 17.7 miles (which was derived under repeatedly conservative assumptions) is remarkably comparable to the mileage within the beat operating as usual. Considering the increased visibility of units responding to calls (rather than performing routine patrol), one may surmise that perceived patrol levels should not be markedly decreased (and may actually be increased) within the reactive beats.

A summary of the conclusion of this section is as follows:

> During evening summer-month tours, when citizens' opportunities for assessing police presence are maximized, the miles travelled by plainly marked police vehicles within the reactive beats to and from calls for service is greater than the miles that would have been travelled on routine preventive patrol by a police vehicle operating as usual. Using reasonably conservative assumptions, the former figure is about fifty percent greater than the latter. Moreover, the

mileage driven in a reactive beat by responding units is comparable (within about twenty percent) to the total mileage that would have occurred in that beat operating as usual. Coupled with the facts that units on calls for service travel faster than units on preventive patrol and that units responding to calls within the reactive beats used siren and/or lights at least five times as frequently as other units, one is led to conclude that the experimental design should not be expected (in itself) to reduce greatly citizen perception of police presence within the reactive beats.

IX. RESPONSE TIME

The technical report emphasizes the fact that response time did not vary (according to the measurement techniques used) by type of beat (reactive, proactive, or control). The dangerous implication to draw here is that patrol coverage could be removed from an arbitrarily selected region of a city and that region would suffer no degradation in response time. Obviously, this is *not true* in most situations. This section presents an examination of, first, the travel time component of response time (again using a back-of-the-envelope model), then the other components (to the extent possible), and finally the data collection procedures used by the researchers.

A. *Travel Time Model*

The technical report states:

> It was originally expected that response time to calls in the reactive beats would be greater in both time and distance when compared to proactive or control beats. This expected difference did not occur. The reasons for this lack of difference are unclear (Kelling et al., 1974:496).

However, using simple back-of-the-envelope models, it can be shown that for *this particular experimental design*, one would not expect travel time or distance to be significantly increased within the reactive beats.

Suppose the idealized square reactive beat depicted in Figure 2 is again considered. If a regular patrol unit operated within the region, patrolling the entire beat uniformly (and following assumptions (2) and (3) of section VIII.C), then the unit (when travelling to a call for service within its beat) will travel an average of one-third of the east-west beat dimension plus one-third of the north-south beat dimension. Thus, the average intra-beat response requires $\frac{1}{3}B + \frac{1}{3}B = \frac{2}{3}B$ miles of travel distance. Similar reasoning shows that for dispatching strategies typically used by police dispatchers, a unit travelling into a beat from an adjacent beat must average twice that travel distance or $\frac{4}{3}B$ miles.[31] If the beat unit is unavailable for calls-for-service (busy) a fraction of time b, then a reasonable estimate of the average travel distance (considering both *intra*beat and *inter*beat responses) is

$$\bar{d}_{REG} = \frac{2}{3}B(1-b) + \frac{4}{3}B(b) = \frac{2}{3}B + \frac{2}{3}Bb \qquad (7)$$

Now consider the experimental situation for reactive beats, as diagrammed in Figure 2. As argued for equation (4), the average travel distance from the perimeter to the call for service

(within the reactive beat) is $\frac{5}{6}$ B. Assuming routine dispatch procedures,[32] response by another unit (in an adjacent beat) would require an average travel distance of $\frac{4}{3}$ B. Thus, again assuming a fraction of time busy equal to b, the average travel distance to calls for service from the reactive beat is

$$\bar{d}_{REACTIVE} = \frac{5}{6} B (1-b) + \frac{4}{3} B (b) \tag{8}$$

$$= \frac{5}{6} B + \frac{1}{2} Bb$$

To apply equations (7) and (8) in Kansas City, estimates of B and b are required. From previous discussions, $B \simeq 1.5$ miles. For an estimate of b, the fact that "... forty percent of all calls for service were handled by officers assigned to beats other than those in which the calls originated" (Kelling et al., 1974:496) is used. Thus, b is estimated to equal 0.40.[33] Using these numbers, one has

$$\bar{d}_{REG} \simeq 1.40 \text{ miles}$$

$$\bar{d}_{REACTIVE} \simeq 1.25 + 0.30 = 1.55 \text{ miles} \tag{9}$$

Thus, the average travel distance experienced by a "regular beat" ($\bar{d}_{REG}$) is not even ten percent less than that predicted for a reactive beat ($\bar{d}_{REACTIVE}$).

This idealized model does not take into account the effects of the nonsquareness of beats, distinct streets and blocks (which prohibit travel through the middle of blocks, for instance), and barriers to travel (such as parks, cemeteries, golf courses, all of which occupy area in the experimental region). An analysis of the nonsquareness of beats was performed, and it was found that the "typical" beat in the experimental region was 1.6 times as long as it was wide. This increases average travel times by about three percent (Larson, 1972:ch. 3). The effect of distinct streets and blocks is to increase average travel distance by about one block length (approximately 0.1 miles in Kansas City). The effects of barriers to travel could be expected to increase average travel distance by another one or two percent. Summarizing this, we will multiply the results of equation (9) by a five percent factor to account for nonsquareness of beats and barriers to travel and we will add to that 0.1 mile due to the distinctness of streets and blocks. The final estimate of the two travel distances then becomes

$$\bar{d}_{REG} = 1.47 + 0.1 = 1.57 \text{ miles}$$

$$\bar{d}_{REACTIVE} = 1.6275 + 0.1 \simeq 1.73 \text{ miles} \tag{10}$$

The 1.73 figure checks well with the average figure of 1.733 recorded by participant-observers in approximately 600 responses in all three types of beat (Kelling et al., 1972:483). However, this reviewer could not understand the report's finer breakdowns by "reactive," "control," and "proactive." Were the finer breakdowns by *type of unit* or *type of beat* generating the call? They should have been by type of beat since it is citizen-specific inequities in the accessibility to police service that are sought. The technical report, while vague about this important point, hints that the finer categorization is by *unit*.[34] The two travel time figures (by beat and by type of unit) are by no means equivalent since about forty percent of

responses are out-of-beat responses, thereby taking one type of unit (say, a proactive unit) and dispatching it to a different type of area (say, a reactive area). Considering the physics of the situation, it would not be surprising to find that the travel times were more evenly distributed by type of unit than by type of beat. The participant-observer data reveals that the reactive travel distance (however categorized) is just about one percent above the region-wide average, the control travel distance about one-and-a-half percent above, and the proactive about two-and-one-half percent below. The model suggests that, if these data were categorized by beat type rather than unit type, the reactive beat travel distance would be about ten percent above the control beat travel distance. Still, even a ten percent difference is remarkably small, and is conditioned on the very particular design of the Kansas City experiment. Other cities should not expect little or no change in travel distance if they arbitrarily remove units from areas.

B. System-Wide Response Time

An analysis of other components of response time is made difficult by scanty reporting in the technical report. It is stated that the participant-observers measured a median time of exactly 2.00 minutes between receipt of a dispatch order and ". . . the time the car began its actual response to the call" (Kelling et al., 1974:484). Presumably this time is incurred by low priority calls that are stacked on cars already busy. (Again, there is a lack of description of the dispatch process.) But this 2.00 minute figure is said to apply exactly to each of the three experimental conditions: reactive, control, and proactive. The likelihood of this occurring in 600 samples, even if the true median in each of the three cases is 2.00, is very small.

Average times from receipt of call until arrival at the scene hover around 6.4 minutes, according to the participant-observers, with no statistically significant differences among experimental conditions. (Again, the method of data categorization is not known – whether by beat or by car.) For instance, the report states, "Although proactive beat cars took the longest to arrive at the scene of the incident (about 6.55 minutes) and control beat cars the shortest (6.26 minutes), the difference is only about twenty-nine seconds" (Kelling et al., 1974:485). (Note the error in calculation here: the two times differ by 0.29 minute, or $60 \times 0.29 \simeq 17.4$ seconds – not twenty-nine seconds.) This reviewer finds it very unusual that the proactive condition has the largest time until arrival of 6.55 minutes. If the categorization is by beat, one would expect the travel time to be least in proactive beats due to the greater density of patrol units there. This would be especially noticeable if the closest car in the proactive beat "volunteered" to high priority calls within the beat.[35]

The estimates of response speed in the report were obtained somewhat loosely by asking the participant-observers to rank the speed on a scale ranging from "slightly slow" to "speed limit" to "slightly fast" to "moderately fast" to "very fast." Over all three experimental conditions, the average rating landed about half way between "speed limit" and "slightly fast." Given the uncertainties of the observers' knowledge of speed limits on all streets and interpretations of the other four speed indicators, it is not clear why the researchers did not simply compute the effective speed of response. This would be, roughly,[36] the average miles per response divided by the average time of response. The average distance per response is about 1.73 miles. The average time is about 6.4 minutes minus the average time from receipt of call until start of call. For the latter, the authors give the serious critic further problems by reporting only the median (2.00 minutes, as discussed before), and not the mean. For the sake of completing the calculations, the two-minute figure is used, thereby estimating travel time, per se, to average 4.4 minutes. Then, the average effective speed of response is

$$\frac{1.73 \text{ miles}}{4.4 \text{ minutes}} = 0.393 \text{ miles/minute, or}$$

23.6 miles/hour. If the larger figure of 6.4 minutes is used, the computed response speed is reduced to 16.2 miles per hour.

In analyzing response time, the authors also performed a limited sample of citizen attitudes. Of the 600 participant-observer sample calls, 581 generated enough data to allow a follow-up mailed survey. Of these, 170 (or 29.3 percent) were completed and returned. Here the report becomes confusing: "Citizens were asked to estimate the length of time they spent talking to both the police department telephone operator and to the police dispatcher." In virtually all departments that this reviewer has visited (though Kansas City is not included in this group), the citizen almost never speaks with the dispatcher, and instead speaks only to the operator.[37] The report needs to be clarified at this point for readers elsewhere in the country. The citizens' estimates of time spent with the telephone operator range from 1.82 minutes (control) to 4.26 minutes (reactive) (Kelling et al., 1974:491). The latter figure is huge, being about three or more times that measured by this reviewer in three other cities. Callers from reactive areas are also said to estimate mean time spent with the dispatcher to be 3.71 minutes. Whatever the definitions of telephone operator and dispatcher, the two figures add up to a whopping 7.97 minutes spent talking on the telephone (as estimated by the sixty-seven city respondents living in reactive beats). If these perceptions bear any resemblance to reality, what happens during all this time? The authors offer no explanation. For some reason, callers from the control beats spend only fifty percent as much time on the telephone. Yet the authors state that the differences by experimental condition are ". . . not statistically significant." Is this due to small sample size? If so, why include these data in the first place?

Here again, if the authors wished to study other components of response time (in addition to travel time), why did they rely on a small number of citizens' recollections rather than on a significant sample of dispatch records?

C. Data Collection Procedures

The entire section on response time is marred by several problems in the way data were collected. First, the participant-observer sample of 600 only allowed about two hundred samples per experimental zone. Second, the mailed survey only produced about sixty per experimental zone. Thus, sample sizes were too small to draw general conclusions. And in the citizen survey, data estimates were asked for average response times that could have been calculated in more precise ways.

When broken down by type of beat, the summary results were often ambiguously displayed – by beat or type of unit? If type of unit, many of these charts in the response time chapter are of very limited value.

As discussed in Section VI, the participant-observers selected their patrol units and beats so as to maximize the number of calls for service and other police activities manned during an eight-hour period. This calls into question virtually all the results of the response time analysis, which the reader is led to believe (until he scrutinizes the next chapter) is based on a strictly random sample. High activity beats typically have a greater fraction of cross-beat dispatches and, accordingly, a higher travel time. This effect of high workload is somewhat offset by the usual smaller size of high activity beats.

In addition, as the workloads of units become higher, the travel times (while increasing) become less and less dependent on the particular beat configuration or spatial allocation of the

units; at the same time, the differences in travel times among the units tend to become smaller. Thus, biasing trends are apparent whose net effects are complicated to predict, the result being a serious contamination of the response time sample.

X. SUMMARY AND CONCLUSIONS

In this review, many issues were deliberately ignored. Foremost among these is the "before and after" crime statistics presented in the final report. Here the researchers found virtually no statistically significant differences in crime among the experimental areas. While the approach itself could be questioned – primarily on the grounds of small sample size – this review focused instead on analyzing the physical operation of the patrol force and the larger police emergency response system. This analysis has shown, in this reviewer's opinion, that the operational consequences of the experimental design were (1) not adequately considered by the researchers, and (2) so different from those expected, that one must question the validity of the design as a means for greatly reducing patrol levels in a subregion. If it is true that patrol visibility (as measured by frequency of passings of police vehicles or by some other equivalent measure) did not differ by the large amounts desired by the researchers (especially at the lower end), then it is not surprising that no statistically significant results are found – no matter how elegant and sophisticated the statistical procedures may be.

A summary of the key points of the review is:

1. The researchers failed to describe dispatch procedures in Kansas City, and it is well known that dispatchers play a key role in determining how units spend their committed time. Dispatching procedures affect many of the performance measures that bear on the Kansas City Experiment.
2. The researchers failed to describe adequately the manning levels in the experimental area for time periods prior to the conduct of the experiment.
3. On repeated occasions, the researchers relied heavily on performance measures as perceived by citizens or participant-observers. While the perceived values are useful, the actual data would have been easy to collect and equally as useful (in some cases more useful) than the perceived values. Lack of reliable data describing call-for-service volume, travel times, on-scene service times, and time-of-day variations in particular are troublesome.
4. The participant-observers' method of choosing beats and cars, favoring those with heavier workloads, biased the results of the travel time analysis (and might have biased the analyses of police-citizen encounters and police noncommitted time).
5. The range of preventive patrol frequencies experienced in the Kansas City Preventive Patrol Experiment was not large enough to encompass the range experienced in many other cities. As shown by a simple mathematical model, even a doubling or tripling of patrol effort – as was done in the proactive beats – does not adequately reflect routine levels of patrol experienced in other cities.

6. The integrity of the experimental conditions is seriously brought to question by the large number of patrol-initiated activities in the reactive beats. This number actually increased during the experimental year, thereby casting doubts on the lack of presence of patrol units in the reactive beats.

7. The integrity of the experimental conditions is further damaged by the freedom and lack of accountability associated with the specialized units in reactive beats. Particularly troublesome is a lack of control on the activities of the tactical units.

8. A significant visible presence in the reactive beats was provided by units responding to calls for service in those beats. This presence was created, in part, by much more frequent use of multiple units responding to calls in reactive beats and by a five-fold increase in the use of sirens and/or lights. The experimental design, by its very construct, resulted in a total mileage travelled in reactive beats during high workload periods comparable to that experienced prior to the experiment (summing the totals of mileage associated with calls for service and preventive patrol). This fact, coupled with the higher visibility of a unit responding to a call for service (compared to one performing preventive patrol), leads us to conclude that during high workload periods, the experimental design should not be expected (in itself) to reduce greatly citizen perception of police presence within the reactive beats.

9. While the researchers claim to be surprised at the lack of significant difference in travel distance or time (by experimental conditions), a simple model demonstrates that one should not expect a marked increase in travel distance in the reactive beats. However, this result pertains only to the particular experimental design utilized in Kansas City. An arbitrary removal of patrol officers from certain areas could in many instances increase travel distance to those areas by a significant amount.

While the Kansas City Experiment may have fallen short in its attempt to prove or disprove the general value of preventive patrol, it offers an unexpected by-product that should be useful to patrol administrators. It implies that if conditions warrant a change in the spatial deployment of units within a confined region such as a precinct, district, or division, then if procedures are followed that are similar to those used in Kansas City, such redeployments can be made without suffering marked degradations in service (either actual or perceived) in the depleted regions. In other words, a roll call sergeant or higher level police planner could on any given tour of duty assign two or more units to some beats (proactive), one unit to other beats (control), and no conventional units to the remaining beats (reactive). Changing crime patterns and other factors affecting public safety would seem to motivate the need for such flexible and focused deployments.

Following the Kansas City experience, the following would seem to be reasonable procedures to follow in implementing such flexible deployments:

1. *Sufficiency of Total Patrol Resources.* Based on analyses of the demands for patrol services throughout the region, a sufficient number of units

should be fielded so as to keep queuing delays at the dispatcher's position (due to all units being simultaneously busy) below some target value.

2. *Patrol Resources in Close Proximity.* Each depleted beat should share at least one boundary with a heavily manned beat (and perhaps one with a regular beat).
3. *Patrol of Common Boundaries.* At least one patrol unit in a heavily manned beat adjacent to a depleted beat should be directed to patrol near the common boundary between the two beats. The dispatcher should be made aware of the identity of this unit.
4. *Spatial Dispersement of Depleted Areas.* No two depleted areas should share boundaries in common.
5. *Patrol-Initiated Activities.* Units patrolling near the borders of the depleted areas should be encouraged to undertake patrol-initiated activities on the borders of (or even within) the depleted areas when, in the judgment of the officers, conditions justify such activity.
6. *Other Operations As Usual.* Traffic, tactical and other special units should not be affected by the change in patrol deployments. In particular, they should not be discouraged from entering the depleted areas.
7. *Dispatch of Multiple Units.* Due to the lack of conventional patrolling in the depleted regions, the practice of dispatching multiple units into these regions may have to be increased in order to guarantee the safety of the responding officers and the person(s) reporting the incident.
8. *Increased Use of Sirens and Lights.* To take maximum advantage of the limited time within the depleted areas, sirens and lights should be used more frequently than usual (in order to increase citizen awareness of police presence).

Many police departments perform limited but unplanned versions of the Kansas City Experiment every day. This often occurs when too few officers report to roll call to field all of the cars in the district, and the roll call sergeant may decide to leave certain beats uncovered (not patrolled) on that tour. If one beat has recently been reporting unusually high crime rates, the sergeant may assign two or three cars to the same beat. Other beats, usually the majority, are assigned one car each. Thus, in a sense, reactive, proactive, and control beats are in use continually in many cities. It would be surprising if citizens or criminals, for that matter, could detect these changes from the one-car, one-beat principle, provided that the selection of beats for each condition changes frequently (and obviously is not publicized). However, following the Kansas City experience, in order to minimize any degradation in service in the depleted regions, it would seem necessary to follow most (if not all) of the eight suggestions above when implementing such deployments.

The effort of the researchers associated with the Kansas City Preventive Patrol Experiment should be commended for bringing the scientific method to bear on the study of police activities. It is hoped that this review and other reviews of the experiment will be seen in the light of encouraging this point of view and strengthening future such experiments.

APPENDIX 1

SUMMARY OF DEFINITIONS OF TECHNICAL TERMS
(in order of appearance in text)

N	Number of patrolling vehicles in a beat.
s	Effective speed (in mph) of a unit while patrolling.
b	Average fraction of time each unit is busy (i.e., not performing preventive patrol).
K	Number of different levels of preventive patrol given to the street segments in a beat.
e_k	Relative amount of patrol coverage given at the k^{th} level of preventive patrol ($k = 1, 2, \ldots, K; 0 \leqslant e_k \leqslant 1$).
ℓ_k	Number of street miles given the k^{th} level of coverage (within a beat).
L	Effective patrollable street miles in the beat (a weighted sum of actual street miles, where the weights are the coverages e_k).
f_k	Patrol frequency (in passes per hour) of a point receiving level k coverage.
D	Total (unweighted) street mileage in a beat.
f	Beat-wide (weighted) average frequency of patrol passings.
B	Dimension (in miles) of a square beat.
$\bar{d}$	Average distance travelled within a reactive beat per dispatch into a reactive beat.
N_{CFS}	Average number of calls for service from a beat per eight-hour tour.
N_{MI}	The average mileage travelled within the reactive beat (on calls for service) per eight-hour tour.
N_{CARS}	Average number of cars responding to each call for service.
$\bar{d}_{REG}$	Average travel distance to an incident in a regular beat (i.e., a beat operating as usual – prior to the experiment).
$\bar{d}_{REACTIVE}$	Average travel distance to an incident in a reactive beat.

APPENDIX 2

SUMMARY OF RESULTS OF EQUATIONS

Equation

(1) Provides a formula for computing L, the effective patrollable street miles in a beat.

(2) Predicts frequency of preventive patrol passings in terms of relative patrol coverages, number of patrolling units, effective speed of patrol, fraction of time not patrolling, and the effective patrollable street miles in a beat.

(3) Predicts frequency of preventive patrol passings at a randomly selected point in a beat, the probability of any particular point being proportional to patrol coverage there.

(4) Computes average distance travelled within a reactive beat per dispatch into a reactive beat (assuming a square beat and a reactive unit patrolling the perimeter).

(5) Estimates the average mileage travelled within the reactive beat (on calls for service) per eight-hour tour, assuming one car per response.

(6) Estimates the same as equation (5), allowing multiple car responses.

(7) Predicts the average travel distance to an incident in a regular beat (i.e., a beat operating as usual – prior to the experiment).

(8) Predicts the average travel distance to an incident in a reactive beat.

(9) Computes numerical values of Kansas City travel distances by employing equations (7) and (8).

(10) Final estimate of the two travel distances.

FOOTNOTES

[1]The work on this review was supported by Public Systems Evaluation, Inc. (PSE), 675 Massachusetts Avenue, Cambridge, Massachusetts 02139. The author wishes to thank Joe Green and Mark McKnew (both of PSE) and Dr. Jan Chaiken (of the Rand Corporation) for detailed discussions and comments on an earlier draft.

[2]In the guidelines issued to personnel in the experimental area (October 2, 1972), "perimeter" is treated as follows: "Cars assigned to Reactive Districts may patrol the perimeter streets of their district or an *adjoining* portion of a proactive district." Here "beat" and "district" are used interchangeably.

[3]As demonstrated in Figure C-1 of the Report, the reactive beats occupied the four corner positions of the experimental area and the center position.

[4]There are many other activities also, as aptly demonstrated in the technical report. These include patrol-initiated activities, (e.g., car check, building check, car chase, assist motorist), transporting persons or papers, court appearances, errands, etc.

[5]Most of the minor miscellaneous activities can also interrupt routine preventive patrol, although most police departments consider many of these activities to be part of preventive patrol.

[6]This is discussed in detail in Section VIII.

[7]According to the report, ". . . within the South Patrol Division's twenty-four-beat area, nine beats were eliminated from consideration as unrepresentative of the city's socio-economic composition" (Kelling et al., 1974:27). No statement is made that suggests that the nine eliminated beats are isolated – in a dispatching sense – from the other fifteen.

[8]Given the power, independence, and discretion of dispatchers in many cities, they can independently affect the experiment, if not closely monitored. The answers to many of the above questions may vary by dispatcher and may be influenced by the response to the experiment of dispatchers in general.

[9]The only significant shortfall occurred with support officers on the third watch, which averaged 3.88 support officers, a shortfall of 1.12 officers.

[10]Here, too, we run into definitional problems since not all of the counts on page 50 refer to calls for service. The table on page 50 is labelled "all other activities," which according to the authors are "primarily calls for service" (Kelling et al., 1974:48).

[11]As discussed elsewhere in this review, the authors fail to make use of actual dispatch data that could yield precise estimates of on-scene service times (and other times).

[12]Police-citizen encounters could be affected negatively by the increased frequency of cross-beat dispatches in heavy workload areas. This increases the chance that the officers responding to a police incident in a particular beat are not normally assigned to the beat and thus are not that familiar with local situations requiring police service.

[13]Certain simple corrections have to be used in order to obtain a good estimate. Chief among these is the mileage travelled on calls for service.

[14]From the limited data that this receiver has seen, New York is not atypical in patrol mileage driven per tour. Patrol vehicles in St. Louis, for instance, typically total fifty to sixty miles per eight-hour tour. However, these figures include travel to and from the station house (which occurs frequently in St. Louis) and miles driven on calls for service, which (as we will show later) could bring the preventive patrol miles down to forty, thirty, twenty miles (or less).

[15]According to the charts on page 501, about forty percent of a car's noncommitted time is spent stationary, about forty percent mobile, and about twenty percent "contacting personnel in field." Dividing the last category evenly between stationary and mobile, we see that it is reasonable to assume that roughly fifty percent of a car's noncommitted time is mobile.

[16]The random selection gives greater weights to the more heavily patrolled areas (in direct proportion to the relative coverages given those areas).

[17]Evidence on pages 38 and 39 of the technical report seems to support a figure of this approximate magnitude. In the control beats, citizens thought they saw a police vehicle more than once a week but considerably less than once a day. Judging from the linear scales displayed on those pages, it appears that citizens saw a police vehicle once every three or four days. If the citizen's location is selected somewhat in the same way as our weighted average for patrol frequencies, then (applying our calculations) he sees approximately one passing per twelve to sixteen actual passings. The actual chance that he sees a car while it passes on preventive patrol may be somewhat less due to the fact that the citizens' observations included cars responding to calls for service. These low figures are understandable considering that (due to equal manning around the clock) a disproportionately large number of passings occur when citizens are asleep, and, therefore, not aware of them.

Given the patrol frequencies that apparently occurred in Kansas City, one may also question the lack of precision available in the responses from citizens who were asked for perceptions of patrol frequencies. For instance, the single response category (in the citizens' survey) of "more than once a week" (but less than once a day) would allow a change in patrol frequency by a *factor* of nearly seven before the survey instrument would detect the change.

[18]This reviewer could find no definition for "E-calls" in the report.

[19]"Any time a reactive car enters his district on an activity which is not the direct result of a call from the dispatcher, it will be accurately recorded on the activity log with a brief explanation as to the nature of the activity" (Kelling et al., 1974:E-1).

[20]Later in this review we question whether some of the reported results are specific to *beats* or *cars*. In the case of patrol-initiated activities, however, there is no ambiguity; the report emphasizes the fact that these data are beat-specific.

[21]According to the technical report, "Data were collected for a nine-month period (January through September, 1973) and compared with the corresponding period for 1972" (Kelling et al., 1974:53). Thus, when we say "1972" or "1973" we are referring to the first nine months of each year. The 1972 data are likely to be slightly contaminated due to the false start of the experiment on July 19 and lasting through mid-August.

[22]In 1973, the control beats experienced a thirty percent increase in patrol-initiated activities and the proactive beats a forty-four percent increase.

[23]The average travel distance predicted by the model does not depend on whether one, two, three or four sides of the perimeter are patrolled. In fact, certain sides may be patrolled more heavily than others, and if the other model assumptions hold, the average travel distance remains unchanged.

[24]Given the officer's identity with the beat and his desire to retain a presence there, once he enters on a call for service, the assumption of exiting in a straight line would tend to underestimate the distance travelled within the beat.

[25]Complications such as nonsquare beats, barriers to travel, and one-way streets would only tend to increase $\bar{d}$, thereby making the calculations reported here (assuming square beats and no barriers or one-way streets) conservative. In Section IX it is argued that these complications could be expected to increase travel distance to an incident by about 0.17 mile. In this section, aimed towards pointing out how large the mileage travelled within the reactive beats must have been, error will tend to be on the conservative side – meaning underestimating reactive beat travel distances.

[26]See, for example, the twenty-four-hour-activity profiles on pages 132, 133 and 168, Larson (1972).

[27]When workloads are high, it is not unusual for units to be dispatched to beats that do not have any common boundaries with their own beats. This forces them to travel through one or more other beats to get to the beat of the incident. If a reactive beat is one of the "pass through" beats, patrol visibility is increased there. Given the lack of details of the dispatching procedures in Kansas City, it is very difficult to estimate the frequency of occurrence of such dispatches. However, at workload levels of fifty-six percent or higher, one would expect that at least a few percent of dispatches (say three to five percent) were of this type.

[28]With probability $(1 - b)$, the beat unit is available and its travel distance to the incident is $(2/3)B$ (using assumptions and methods discussed earlier). This figure is not multiplied by 2 since it is assumed that the unit resumes preventive patrol from the scene of the incident. With probability b, an out-of-beat unit must be dispatched to the incident; this unit will travel about $(4/3)B$ units of distance within the beat. Adding the two figures, each weighted by the respective probability of occurrence, $(2/3)B(1 + b)$ is obtained.

[29]A patrol unit fielded on an "average tour" (as sampled by the researchers) experienced a workload of forty percent committed time (Kelling et al., 1974:501). It would appear to be conservative to increase this figure by only fifty percent (from forty to sixty percent) during the heaviest workload hours.

[30]See Section VII.B.

[31]Larson, 1972:ch. 3; assuming strict center-of-mass dispatching.

[32]Regular police cross-beat dispatch procedures are assumed since the report states that dispatchers were told to operate as usual. As stated in Section III, at least part of the dispatching process had to be affected by the experiment. The assumption made in deriving the cross-beat average travel distance into a reactive beat is that the dispatcher's car selection policy is "strict center-of-mass," the same assumption used to derive $\bar{d}_{REG}$ and one that closely

models dispatcher behavior in many cities (Larson, 1972:ch. 3). Since the average number of cars responding to calls for service into the reactive beats was 1.58, there was most likely considerable "volunteering" based on the perceived nearness of the volunteering unit to the incident. Such behavior on the part of the responding units would tend to make the average cross-beat travel distance somewhat less than $\frac{4}{3}B$, as predicted by the strict center-of-mass dispatching assumption. Thus, the formula in equation (8) for the average travel distance to calls for service from the reactive beats could slightly overestimate the true value. This, in turn, would make the point of this section even stronger, namely that little increase should be expected for travel distances into the reactive beats (given the particular experimental design in Kansas City).

[33] This represents a different definition of "time busy" than used previously for preventive patrol. This busy time is time not available to respond to calls for service rather than time not performing preventive patrol. Various committed and noncommitted time activities make the two definitions somewhat different.

The argument which allows the inferring that $b = 0.40$ from the fact that cross-beat dispatches equalled forty percent of all dispatches is formalized in Larson (1972:243–51).

[34] The distances from the location where the call was received to the location of the incident were 1.75 miles, 1.76 miles, and 1.69 miles for "reactive," "control," and "proactive," respectively. The levels are not specifically assigned to beats or to units. However, when discussing the three figures, the report suggests that the categorization is by unit: "While proactive *beat cars* tended to be somewhat closer to the incident, the differences were so small as to be inconsequential" (emphasis added) (Kelling et al., 1974:484).

[35] The same travel time reduction effects could be obtained from intelligent use of an automatic vehicle locator (AVL) system.

[36] Neglecting certain effects such as those due to time and distance truncations.

[37] Only in very high priority situations (e.g., robbery in progress, gunfire) are the citizens' calls linked in directly to the dispatcher, thereby bypassing the operator.

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